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The study of nature – A seamless tapestry

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I would like to thank the Royal Society of Western Australia for their recognition of my research contributions. In a sense I am astonished that these intellectually enjoyable activities, undertaken out of my own curiosity, should be acknowledged by the award of the prestigious Royal Society Medal. I also thank Professor Alan Robson, Deputy Vice Chancellor of the University of Western Australia for personally presenting the medal. The ready and active participation of graduate students and colleagues ensured the success of all projects and I thank them sincerely.

Introduction

From time to time there have been inferences that my interests were too broad, my application undirected or that I was excessively opportunistic in my research. For example, when I was still involved with frog ecology and beginning studies of macropods, I was told by one worker that I could only expect one good idea in my life and I should stick with it. On a later occasion, another suggested that I had so many ideas that I must have stolen them. I take my cue for this lecture from these comments and briefly review and interpret the findings on the topics investigated by myself and co-workers to show that I did indeed pursue a theme of adaptations to aridity which led along many unexpected pathways.

My research has been devoted to answering questions posed by problems arising from field observations. These were initiated and conditioned by my experiences as a child when I lived within walking distance of the bush. From the time I could walk a reasonable distance, I was taken to the bush on most weekends by my grandfather and natural history mentor. I have an abiding memory of puzzlement at the contrasts between summer and winter and of wondering how so many animals managed to persist under such conditions. Briefly I was, and still am, interested in understanding the world about me. Natural History observations exposed the changes that took place but were unhelpful, except in so far as they posed testable hypotheses, in resolving the underlying mechanisms of persistence of animals or how such mechanisms may have evolved. In retrospect I now see these twin topics as motivating my curiosity about species, their adaptive traits and evolution.

It was only after war service and the benefits of the Commonwealth Reconstruction Training Scheme that I was able to gain the necessary technical skills with which to pursue my childhood interests. Parenthetically, I may comment that upon my appointment to the Zoology Department I was asked by Professor Waring what I intended to do as research. My reply embraced the points mentioned above; as soon as I mentioned evolution his comment was 'It's all known, there's nothing to research in that'. Fortunately he did not attempt to stop me trying.

In what follows, I set out the groups of animals on which work was conducted to show the scope of the studies and to illustrate two things;

1. The interdependence of studies when interpreting observations.
2. The relevance of these studies to a wider understanding of local biology,

and how the studies led into societal issues such as conservation. I devote much space to discussing work on identification of species of frogs, the group I had chosen for comparative studies of their adaptations to aridity. But, comparative studies depend on objective identification of the species being compared, and at the start of the investigation it was not clear that systematists used objective criteria to identify Western Australian species of frogs. I present the sorting out of the problems of species' identification in some detail because it illustrates the difficulties associated with obtaining an adequate, objective, and biologically-meaningful systematic and taxonomic knowledge on which to base comparative studies of adaptation to aridity. It also explains why I have in many quarters been seen as a herpetologist rather than as an ecologist.

Frogs

Harry Waring had come to Western Australia to study marsupials, especially their reproductive physiology, and he chose to work with the quokka *Setonix brachyurus*. This species was common on Rottnest Island, and it and other marsupials that Waring and his students studied were morphologically distinct, readily identified, and posed no problems when establishing the species status of the population upon which the work was being done. This was not necessarily the case with other easily collected animals.

Prior to coming to Western Australia, Waring had worked on colour change using the South African clawed toad *(Xenopus laevis)* as an experimental animal and he was naturally favourably-inclined to frogs as experimental animals. Moreover, while there were many named species of frogs occurring around Perth, it was not apparent that the named kinds had the same status as species that was enjoyed by marsupials. In particular, the last two workers who had reviewed Australian frog taxonomy (Loveridge 1935, and Parker 1940) held markedly different views. This was unfortunate because I had formed the view that because of the unprotected nature of their skin, frogs should be useful organisms in evolutionary studies as it was believed, at the time, that speciation in some of the biota had been promoted by a great aridity in the geologically recent past (Crocker & Wood 1947). If this were so, then adaptations to drought and their evolution might be linked to the same phenomena. But, it was necessary to first be certain that named species had a biological reality in the field and this was certainly not so as the following illustrates

In his revision of the Australian frogs of the family Leptodactylidae Parker (1940) used the flash colours in the groin to distinguished the two *Crinia* species, *georgiana* and *signifera*. These flash colours were said to be strongly developed and fast in preserved specimens of *C. georgiana* or absent or fugitive in *C. signifera*. However, when preserving specimens I had noticed that colours remained fast in either formalin or alcohol when live specimens were preserved but faded when specimens were dead before preservation. On the other hand, Loveridge (1935) believed absence of colour was indicative of *C. georgiana* whose dorsal pattern was rugose or with a few scattered warts and raised undulating glandular folds. In contrast, he classified *C. signifera* into two sub-species, *C. signifera affinis* when the back pattern was perfectly smooth without the lyre shape or *C. signifera ignita* in which a lyre-shaped dorsal fold was present. Clearly this suggested that the names assigned by museum taxonomists to species were unlikely to reflect the presence of field entities and gave no confidence that specimens agreeing with literature descriptions could be assigned names which represented biological entities which may show adaptive traits to the local environment.

Fortunately, the biological species concept had recently been proposed (Mayr 1949) and Moore's work showed the genetic inviability and hence the presence of cryptic or sibling species, within what was formerly regarded as *Rana pipiens* (Moore 1946, 1949). I already knew from observations of the many distinctive call types among local frogs that if distinct calls were assumed to reflect a specific mate identification, then putative species could be identified by collecting calling males which would represent different breeding, and hence genetically distinct, entities. The genetic distinctness of these entities could be confirmed by making crosses using Moore's technique of *in vitro* fertilisation, with appropriate controls. Normal development would be interpreted as being indicative of a within species cross, while abnormal development in the experimental cross and normal development in the control would indicate a cross between species. The sorts of abnormalities which occurred with crosses between different call types of *Crinia* were failure to develop beyond early cleavage stages, or embryos that hatched but were headless, haploid or with extruded guts. Clearly, populations used in the crosses and identified by their characteristic calls were not capable of sharing a common gene pool and hence could not be considered as biological species in an evolutionary sense.

However, before an experimental programme could be undertaken, an objective method of recording and measuring of calls was needed. Fortunately, the Australian Broadcasting Commission co-operated by loan of equipment and running a programme on the distinctiveness of frog calls (Anon 1953). I was able to use these tapes to show by means of oscillograph analysis that different calls were distinctive in duration, repetition frequency, and pulse rate. It was now only necessary to build a portable tape recorder and the research could commence. The construction was undertaken by Murray Littlejohn, then a doctoral student, and he was able to show that a number of distinct call types could be identified in the genus *Crinia* and I was able to confirm the species status of these by means of *in vitro* crosses. At the same time Tony Lee confirmed the species status of different call types (Littlejohn & Main 1959) within the genus *Heleioporus*, which are all large burrowing frogs. A new species was also identified within the dry land burrowing frog genus *Neobatrachus*. At this stage, the conflict between Parker and Loveridge on the characters to be used in distinguishing *C. signifera* and *C. georgiana* had been resolved, the species status of *C. glauerti* had been confirmed, and sibling species of *Crinia, Heleioporus* and *Neobatrachus* identified (see Main 1968 for review and literature citations).

I might comment here that the recognition and naming of species using biological characteristics was not readily accepted. The substance of the objections could be reduced to two elements, species were those entities identified by recognised taxonomists, or that species should be only diagnosed by characters that were available to museum curators with access only to preserved specimens.

Once the species status of specimens could be established, it was possible to readily identify calling species in the field and so be certain of the status of collected specimens. Only then could meaningful experimental comparisons be made in the adaptive responses of the species to aridity and drought. This work was done in collaboration with Peter Bentley (Bentley *et al.* 1958; Main & Bentley 1964; Bentley & Main 1972a, b). In the midst of this work on water balance in frogs, lizards, birds and macropods, a physiologist suggested to me that it was quite improper to prostitute physiology as a field study in order to interpret the ecology of species.

The progeny arising from the controls from the *in vitro* crosses of the various call types of *Crinia* were reared to metamorphosis and these showed that the dorsal pattern so emphasised as a taxonomic character by Loveridge (1935) was an inherited trait (Main 1965).

The early work using the mating calls of males to identify species was the subject of much comment, of which the essentials were; we all know that frogs are cold-blooded vertebrates and cannot regulate their body temperature; moreover, call characteristics such as pulse rate and repetition frequency will be dependent, as in all cold-blooded animals, on body temperature; since body temperature cannot be regulated, then calls cannot be as constant and characteristic as is claimed. However, whenever calling males were collected for use in experimental crosses, body temperatures were recorded and when collated these records showed specific seasons of calling and body temperature ranges for each species (Main *et al.* 1959, figs 2 and 3).

These doubts about my field-based approach produced benefits for me because Waring induced J A Moore of Columbia, then a visiting Fullbright Scholar in Australia, and later Th Dobzhansky, also of the same institution, to visit and spend time with me in the field. Waring was also instrumental in having Julian Huxley visit and accompany me in the field. I benefited immensely from these occasions and the opportunities I had to discuss evolutionary problems with them in the field. I doubt that a higher level or more intense peer review could be achieved.

I was funded in this research by University of Western Australia Research Grants, and administrators then, as now, were interested in accountability as the following illustrates. Many stores and fuel outlets in the country would not accept University purchase orders because of the perceived paper work and the delays in obtaining cash recoups. Thus I frequently had to pay cash and obtain a recoup upon my return. On one occasion, after an extensive field trip to the goldfields and pastoral areas, I was told that there were plenty of frogs around Perth and there was no need to travel, as I had done, to distant places to get them. I answered this criticism and was told not to be impertinent. When it comes to rebuking administrators who desire to limit or set boundaries of research, I hope I have not mellowed over time.

The frog work demonstrated the significance of behavioural avoidance of stressful situations as a complement to physiological adaptations. Clearly, the basis of the adaptations had been revealed and further work would involve laboratory analysis and skills which I lacked. The frog work was then reviewed and summarised by me (Main 1968).

Other Species (Reptiles and Birds)

Field work with frogs and macropods is primarily a nocturnal activity, and during the days I took every opportunity to measure the body temperatures of lizards caught in the field. During the 1960's W R Dawson, accompanied by P Licht and V Shoemaker, spent a year in Western Australia studying the temperature tolerances and biology of lizards. The data which I had already collected was assembled, and added to by them, to reveal that each species exhibited an activity pattern related to a specific temperature range (Licht *et al.* 1966).

Prior to this S D Bradshaw had begun a comparative study on *Amphibolurus ornatus* (*Ctenophorus ornatus*) which showed how intricate relationships between diet, water metabolism, electrolyte balance and behaviour led to individual survival and population persistence (Bradshaw & Main 1968).

In a comparative study of three species of chat (*Ephthianura*) which live in progressively drier habitats, G K Williams showed that behavioural and physiological traits changed in a way that paralleled the environmental changes associated with the different habitats occupied (Williams & Main 1976, 1977).

Macropods

While the foregoing research on frogs was proceeding, Professor Waring was in receipt of supporting funds from CSIRO for his studies of reproduction of marsupials. These studies concentrated on the reproductive physiology of the quokka (*Setonix brachyurus*) and the Tammar wallaby (*Macropus eugenii*), and it was during this time that studies on the ecology of marsupials was begun (Main, Shield & Waring 1959). These ecological studies focused on the ability of kangaroos and wallabies to survive periods of drought when water was scarce and available food was of poor nutritional quality. A preliminary study by me showed that the nitrogen levels in the gut contents of the quokka on Rottnest exceeded what could be expected even with the most favourable selection of dietary items, and suggested that the quokka might have a ruminant-like digestive system and thus would be capable of recycling urea. This supposition was confirmed by Moir *et al.* (1956). These observations led to further studies of dietary composition, water metabolism and urea recycling in kangaroos and wallabies.

Analysis of dietary composition was undertaken by G Storr, who used leaf epidermis to identify plant species eaten by the quokka, red kangaroo and the euro (Storr 1964, 1968)

E H M Ealey, then of CSIRO Wildlife Section, had begun a study of the euro (*Macropus robustus*), then regarded as a pest of the pastoral areas in the Pilbara. I was asked for assistance and advice based primarily on knowledge of the ecology of the quokka on Rottnest Island, which I regarded as a model of how a macropod adapted to aridity. I believed this to be so because, in its typical range, the quokka occupied mesic situations whereas on Rottnest its habitat was considerably more arid.

Later, as the work on the euro developed, I was quizzed by the head of the Wildlife Section on what I was doing and where it might lead. I explained to him that I believed that an understanding of the water and dietary needs of the euro were basic to any understanding of the ecology of the species in the arid area in which it occurred. From his comments I was left in no doubt that what I was proposing was a nonsensical vision, certainly lacking in any basis in classical ecology and with no hope of contributing to an understanding of field observations. Moreover, it was likely to be unproductive in the sense that it would not lead to an understanding of how kangaroos could live in such an inhospitable environment and particularly how the species could achieve such a pest status when available forage could not support sheep. In the event financial support continued and led to papers on field ecology of the euro (Ealey & Main 1967), diet (Storr 1968), water metabolism (Ealey *et al.* 1965) and nitrogen requirements (Brown & Main 1967). In essence, this work showed that the euro could supplement the low nitrogen of its diet by recycling urea.

Meanwhile, work continued on other macropods, especially the tammar wallaby whose ability to persist and thrive in dry habitats is well exemplified by its occurrence on East and West Wallabi Islands of the Abrolhos. These studies dealt with the ability to drink sea water

(Kinnear *et al.* 1968; Purohit 1974), recycle urea (Kinnear & Main 1975), and water and electrolyte metabolism (Bakker *et al.* 1983). A general interpretation of this work in terms of nutritional biology of ruminant and ruminant-like mammals was developed (Kinnear *et al.* 1979) and related to niche theory (Kinnear & Main 1979).

These studies were reviewed in the context of native animals as resources (Main 1969), measures of well-being (Main 1971; Bakker & Main 1980) and adaptations to desert conditions (Main 1975; Main & Bakker 1981), and as a factor in the Late Pleistocene extinctions of marsupials (Main 1978). A wider more generalised review (Main 1983) indicated a possible integration of macropod ecophysiology with ecological and evolutionary studies in the context of either the fundamental or realised Hutchinsonian niche (Hutchinson 1957).

The fundamental niche is an n-dimensional space which encloses all the environmental states that a species can tolerate and persist in indefinitely. In the presence of other species with which there are biological interactions, a species occupies a realised niche in which the dimensions are less than those of the fundamental niche. In the context of ecophysiology of macropods, these dimensions relate to heat tolerance (including reflective pelage), water balance, electrolyte metabolism, ability to recycle urea (supplement dietary nitrogen), and behavioural traits related to environmental structure (shelter and cover) which permit trade-offs between attributes of the niche and persistence in the unpredictable Australian environment. In the absence of other interacting species, the realised niche may approximate the fundamental niche. This is important in cases of single species on islands or in reservations where species may persist in quite atypical habitats because they still satisfy the dimensions of the fundamental niche *e.g.* quokka on Rottnest. Furthermore, when species are observed under field conditions, we see only the dimensions of the realised niche which may be a poor basis for prognosis of persistence or success in reservations where the ecosystem is changed and perhaps simplified.

The findings arising from the studies on macropods, birds and reptiles have converged to show that nutrition, dietary composition, water and electrolyte balance coupled with behaviour conspire to provide a suite of labile adaptations which permit survival when the environment in which the animals found themselves offered the scope to display and exploit the evolved traits.

These findings are relevant to conservation and raised questions of what could be retained in reservations. A question I had earlier (see below) approached from the point of view of adequacy of reserve size using macropod species retained on continental islands as a surrogate measure. However, the studies mentioned above raised questions other than simple size as contributing to persistence. For example, habitat structure, composition, senescence and regeneration appeared to be critical to maintain the habitats which gave scope for adaptive traits to be deployed. While these studies indicated requirements necessary for persistence of macropods, they also hinted at another problem, namely, whether management for the successful retention of nominated species (flagship or umbrella species) would ensure that other elements of the biota were also be retained.

Conservation

Like my other biological interests my concern with conservation dates back to my youth and as a member of the West Australian Naturalists Club.

While in the Army I had been stationed on the sand plains west of Dandarragan and had become very aware of the immense floral and invertebrate species richness of these areas. In 1947 I attempted to have extensive sandplain areas reserved but without success. The consensus opinion at the time was that these areas could not be used for agriculture and so would always remain as vacant crown land, and having them reserved would merely preclude other better areas from being considered for reservation. The fallacy of this reasoning became apparent as soon as it was realized that the agricultural potential could be enhanced simply by applying trace elements to the land.

My next direct involvement in conservation occurred when I set a student project on the Western Swamp Tortoise (*Pseudemydura umbrina*). This was taken up by Andrew Burbidge as the topic of a doctoral dissertation. Again, taxonomic relationships were crucial in obtaining a perspective for its conservation (Burbidge *et al.* 1974). Many subsequent observations by Burbidge have led to an understanding of the difficulties associated with conserving small populations of vertebrates.

In the 1950's, I had become associated with the Fauna Advisory Committee, later to become the Western Australian Wildlife Authority, which had the statutory responsibility for nature reserves under the Fauna Act. Initially the committee and the Authority advocated that no interference with reserves equated to good management of them *i.e.* leave Nature alone.

To me, reservations posed several problems;

1 there was no evidence that the size of the reserves being set up would fulfil the purpose for which they were being created especially whether they were adequate in area to maintain the biota included;

2 whether the disturbance resulting from fire and consequent plant successional pattern would permit the persistence of animals for which the reserves were being set aside;

3 whether the changed biotic environment *e.g.* within reserves, would permit the display of evolved traits and behaviour which laboratory and field analyses had demonstrated were a pivotal part of adaptation to the present environment.

At this time, even as at present, the species around which conservation efforts centred were vertebrates, usually marsupials and birds. In the absence of a long record of successful conservation within reservations, there was no way of directly establishing how large an area would retain one or more species of mammal. However, numerous islands around the Western Australian coast retained macropod marsupials since their isolation from the mainland by rising sea levels at the beginning of the Holocene. These islands offered a surrogate measure of the number of species of macropods which could exist on islands of various sizes for up to 11 000 years. Main (1961) and Main & Yadav (1972) argued that these data provided the only evidence for predicting what could be retained in reservations of restricted size, assuming they were the analogue of islands.

The quokka on Rottnest and the tammar on the Abrolhos could persist in harsher, much more simplified environments than that usually occupied on the mainland, provided that the animals could exploit their behavioural and physiological repertoire of adaptive traits *i.e.* the environment remained within the dimensions of the species' fundamental niche. Thus, the prognosis was that the populations would persist in reserves of equivalent size. Unfortunately this first approximation underestimated the significance of the fox.

When setting aside land for reservation, it was hoped that marsupials, with their readily identified public appeal, would act as umbrella species beneath which other elements of the biota (plants and other animals, especially invertebrates) would be sheltered and so retained along with the high profile or principal species.

Notwithstanding the persistence of macropods on islands, it was clear from personal familiarity that the habitats available on an island are quite unlike mainland habitats in which the species now occurs and which were presumably like the ancestral ones. It is conceivable that proximity to the sea, salt spray, and exposure to high winds and storms may have changed the composition of communities. However, reserves in the wheatbelt landscape are also isolated and exposed, and retention of macropods in them was no guarantee that other elements of the biota initially included with them in a reserve would be also retained. This in turn suggested that a reserve management policy based on 'leaving Nature alone' was unlikely to be successful, particularly if macropods, or other charismatic species, were used as umbrella species in the hope of retaining other elements of the biota. Despite these quibbles, the islands around the Western Australian coast do indicate that retention of the original biota is dependent on island area. Large islands such as Bernier, Dorre and Barrow have not only retained macropods but also most resemble the mainland with respect to composition of the vertebrate biota prior to the arrival of the red fox.

The foregoing highlighted the need for understanding the dynamics of populations, communities and ecosystems, and particularly the role of disturbance, especially fire and herbivory, in their maintenance and regeneration. In the absence of the formerly wide-spread burrowing marsupials, rabbits and fire now tend to be the commonest agents causing disturbance and inhibiting plant regeneration. However, invertebrates such as grasshoppers may also exert subtle influences such as inhibiting the post-fire regeneration of jarrah trees by eating seedlings at the cotyledon stage (Whelan & Main 1979). This result emphasised how much the judgment that, 'it is reasonable to assume that insects are much less significant than grazing mammals in determining floristic composition', depends on our level of ignorance. An approach to the management of reservations in the Wheat belt of Western Australia was proposed by Main (1987).

In the early 1970's, I accepted appointments to the Universities Commission, the Council of the Australian Institute of Marine Science, and I was one of the initial three members of the Western Australian Environmental Protection Authority. These appointments precluded extensive field work and my research emphasis changed as other issues emerged.

Important among the emerging issues were:

(a) the potential for climatic change, under the influence of the greenhouse effect as atmospheric composition changed, to alter the structure and composition of biota retained within reserves;

(b) an acceleration of salt encroachment in the wheatbelt landscape;

(c) a developing appreciation of the significance of patches of remnant vegetation in maintaining a hydrological balance (many of these patches may be reservations for conservation purposes);

(d) an appreciation of the necessity to develop concepts which would guide management practices rather than offer universal prescriptions for action.

In a very important sense, these issues all have a bearing on what can be retained within or outside reservations because they determine components of the realized niche of those species which are of concern for conservation. But the problem of establishing what species can persist is experimentally intractable over the period available for meaningful action to ensure retention.

The fundamental niche will only change slowly under the influence of selection; however, the realized niche will change whenever environmental conditions alter. Thus, should changes occur it is unlikely that the present composition will be retained because each species is likely to have a new realized niche resulting in the reorganisation of ecosystems. Notwithstanding these limitations, it seems likely that the insights gained from the studies reviewed above offer useful guidance in each of the fields listed. These have been applied in relation to ecosystem management (Main 1981a, b, c), and management of remnants of native vegetation (Main 1987), retention, significance and problems in retaining of rare species (Main 1982, 1984), fire tolerance of animals (Main 1981b), the role of biodiversity (Main 1992), climatic change and its likely effect on nature conservation (Main 1988), and restoration ecology (Main 1993).

Concluding Remarks

In the introduction, I set out how the lecture was to develop *i.e.* it was to be an illustration of how my natural curiosity posed the way in which questions were asked and how these led to matters of conservation. It is now possible to draw the lessons together and in doing this I wish to emphasise the close relationship of biology, politics and society.

The core of science is; recognise entities; distinguish them from similar entities; classify; explain *i.e.* have an hypothesis and then test it. The first step is to distinguish and describe entities so that others can recognise them. Should recognition not be precise then the use of the entities in experiments *e.g.* taking sibling or cryptic species together as one entity will mean that comparative studies will be handicapped.

But there is a more general point to be made with regard to classification. I have been, or am regarded (classified) as a herpetologist (I admit that being an elected Honorary Life Member of the American Society of Ichthyologists and Herpetologists must colour the interpretation) but the initial choice of frogs for study was my familiarity with them and a belief that they would be a satisfactory experimental animal for study of adaptations to aridity. This last theme is the key to my work on frogs. Moreover, the pursuit of a problem or theme across disciplinary (taxonomic) boundaries *i.e.* a comparative approach, is the only way to test the generality of an interpretative hypothesis which in the present case was that physiology, behaviour in an environment which permitted the display of evolved physiological and behavioural responses provided the basis for survival of individuals and persistence of populations. The survival of organisms during periods of drought and aridity experienced in the past and their capacity to persist in restricted areas and as small populations during such times (including post-fire situations) is likely to have selected traits that are adaptive under such conditions. Study of such past events and population consequences is relevant to conservation where persistence as small populations in restricted areas is the central problem to be solved. But this is an example of seamless Nature; the call now is for teams to undertake interdisciplinary studies in order to solve a problem. The problem may be well defined and pressing; however, there is often an absence of theory on which testable hypotheses can be erected. Additionally, it is often not clear that a need for theory is appreciated, the vision of what is to be studied is purely practical, applied and constrained by utilitarian goals. The current funding sees the study of Nature as being that of patches of fabric which can be stitched or cobbled together to serve utilitarian purposes. This result is the logical end point of the comments received by me and referred to above regarding my disrespect for, and transgression of, the currently accepted disciplinary boundaries, namely that knowledge of the world about us is hopelessly fragmented.

Of course the initial choice of questions to be addressed in my research was not a scientific one. It may be conceived as morally proper to know and understand the surrounding world but the decision of what to study and how to proceed was a personal choice, only the methodology was scientific. Yet, as mentioned earlier, the crossing of perceived boundaries did receive some comment. which can be interpreted in two ways;

1 that the accepted boundaries should be taken as given and not transgressed;

2 the decision by me to try and understand the surrounding world was morally wrong. A proper approach was to stay within boundaries and/or to address problems posed by society *i.e.* deal with applied problems where a need for cross disciplinary work may be demonstrated.

Thus the current approach of assembling teams formalises, in an administrative sense, the personal prejudices so frequently expressed in the past *i.e.* synthesis is only needed when practical solutions demand it. The broad understanding and interpretation is seen as unnecessary; it is only needed to satisfy the pragmatic requirements of cost effective economic progress *e.g.* to ensure 'the sustainable utilisation of ecosystems and species' (Anon 1987).

More broadly, the foregoing is often taken to mean retaining biodiversity (species richness) and ecosystem functions. Taking the ecosystem functions to embrace responses to competition, disturbance, and stress (Grime 1974) and recognising the physiological and behavioural requirements of species for their survival poses the question of the nature of the assumption being made about environmental stability. At its simplest, the assumption is that the present is like the past and the future will be likewise. Historical geological considerations suggest that this is unlikely. Moreover, conditions are changing rapidly with development which is leading to more radical changes than occurred in the past; it is unlikely that the future will be like the present. Thus we are left with the question of whether the physiological, nutritional, and life history and behavioural traits of species (their fundamental niches) can be satisfied in the new environments. These are management problems and successful outcomes depend on decisions being made promptly, but with recognition that adaptive management in the sense used by Walters (1986) may be called for. This is a condition that is rarely met because there is a perception that there should be complete understanding before action is taken or decisions made. This desire for certitude is, however, often eclipsed by a desire on the part of administrators to achieve or maintain status or exercise sheer power which would be entirely appropriate in the jungles of the natural world. The general difficulties experience by ecologists when dealing with decision-makers have been discussed by me (Main 1996). Despite the traumas and difficulties associated with involvement in arriving at, or implementing, decisions it behoves ecologists to become involved in the way their knowledge is used, without their presence when decisions are made ecological knowledge will be misconstrued, misinterpreted or ignored in both conservation problems and environmental issues. I regard discovery, interpretation and application of information as the result of a conscious decision as being the political manifestation of the seamlessness of Nature.

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To the extent that I have succeeded in the pursuit of my vision I have been favoured by luck in being in the right place at the right time. To all those who made it possible, especially my wife Barbara York Main, I am extremely grateful.

It has been wonderful for me to pursue my hobby. I am sure I could not do so in the present economic and funding climate.

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